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Letter to the Editor

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explicating the actual content of the period's science is . . . evident" (p. 177). I invite the experts in eighteenth-century mathematics and physics upon whose shoulders my book is built to read this review, and the analysis in my book that it claims to represent, and then to form their own opinion about which author suffers from a lack of precision in understanding the content of eighteenth-century science.

More examples could be offered, for Feingold demonstrates little ability to escape his own partisan agendas and actually engage with my book's substantive claims. What he disparages as "fashionable trends" of scholarship and "modish line[s] of argument" are better described as broadly supported scholarly interpretations that he simply does not agree with. The careful reader should also note that despite all of his bluster about empirical precision Feingold does not manage to see and record the word "French" in the title of the one book he is most responsible for in his review: mine. That the French focus of my book fell into Feingold's blind spot is not without significance, but lacking the space to unpack this pregnant gaffe I'll simply ask, What manner of careful reading and reporting (not to mention editorial oversight) does the error suggest?

The Newton Wars and the Beginning of the French Enlightenment is the result of over a decade of research, and it contains over five hundred pages of densely worded text supported by almost fifteen hundred footnotes and a bibliography that runs to over forty pages. Yet to Feingold "what is notably absent here" is "a serious engagement with the wide range of issues that impinge on the diffusion of Newtonianism and on the origins of the Enlightenment" (p. 186). If there is a tendentious author in this exchange, I will leave it to readers to decide who it is.

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IN REPLY:

J. B. Shank opens his disgruntled letter with a distortion. He cites part of my review's first sentence—"the engrained notion that Newton's genius can account for the advent of scientific modernity and the subsequent French Enlightenment"—in order to berate me for allegedly espousing this notion, whereas I made explicit in my review that this is the "seriously misguided" position that Shank wishes to refute. His letter makes clear that I did not misrepresent

his intention; so why does he misrepresent me? The answer is simple: like the postmodernist heroes that inspire him, Shank exhibits a fundamental disrespect of texts. He believes that a text—to cite Tzvetan Todorov, paraphrasing Lichtenberg—is merely a "picnic" to which "the author brings the words and the reader supplies the meaning." Hence he feels free to imbue his sources with preconceived conclusions.

Accuracy and scholarship should matter to a historian. Greatly. I also believe that it is incumbent on the reviewer to subject books to careful scrutiny and, when necessary, to expose their authors' ignorance or errors, especially when disguised by a display of false erudition. In my review, however, I was careful to avoid personal reflections and innuendos; Shank apparently does not feel so constrained.

Shank had an opportunity to refute my claims and to demonstrate where I misquoted him or misrepresented his argument or where my contrary evidence is faulty. He chose not to do so, preferring to wave his hands in indignation and blame the editors of *Isis* for not allowing him sufficient space to expose my mistakes. The eleven hundred words he wasted would have been put to better purpose by producing at least a sample of my alleged errors of fact and interpretation.

Shank's letter, however, does provide additional evidence of his preferred practice of misreading and misrepresentation, when marshaling forth two instances purportedly illustrative of my "many misunderstandings" of his book: "On one occasion Feingold appears to agree with me that there was little public debate, saying that 'the immediate and profound impact exerted by the *Principia* on European savants rendered it unnecessary to mention Newton by name when controverting him.'" Anyone would recognize that, far from agreeing with Shank, the very sentence he cites argues *for* the existence of an intense public debate over Newton, albeit without mentioning him by name, for reasons that I explained at length in my review. Shank's unwillingness to appreciate the difference between the two is indicative of his conviction, evident everywhere in his book, that texts are malleable, capable of being made to fit any conclusion.

As for Shank's other claim, that I "muddled" his "painstaking effort to accurately synthesize the technical literature on the development of eighteenth-century mathematical physics," it is he who muddles the technical content and an understanding of the connections between the physical and mathematical aspects of Newtonian force. Or, rather, he could be said to have muddled technical matters if there actually were

any such anywhere in his account. If he had the technical content under his belt, he would know precisely what it is that Leibniz and Huygens misconstrued initially (as shown long ago by the real expert, Tom Whiteside) and why it is significant. Moreover, to claim that he has produced a “fully elaborated” account of the “conceptual and mathematical articulation” of the subject is simply nonsense. Shank shows no understanding of the vast, truly technical literature, both analytical and philosophical (e.g., by George Smith, Andrew Janiak, Niccolò Guicciardini, and others) that has strikingly revealed the proper underpinnings of this complex subject. Citing an authority in a footnote is no indication of understanding. It is not difficult to expose Shank’s multiple failures in this regard. The point I made, and which he finds baffling, should have been easily apparent to anyone who knows: namely, that his discussion considers the mathematics of force to be simple and unproblematic. If that were so—if the mathematical structure of Newton’s ways with the vexed concept of “force” was unproblematic at the time—then those who had an utterly different physical understanding surely would have easily deployed it. In that case “force” would indeed have been divorced from the issue of its character as a physical and metaphysical entity. The literature of the period unequivocally shows this separation to be false and deeply misleading. (For one pertinent example, see Domenico Bertoloni Meli’s examination of Leibniz’s *Tentamen*, which, he has shown, was produced with knowledge of Newton’s *Principia*.) Yet even a cursory glance at Shank’s book will demonstrate that he finds it unnecessary to grasp the many subtleties involved in the deployment of Newton’s new physico-mathematical structure. To reiterate, his protestations notwithstanding, Shank studiously avoids as much as possible dealing with the actual science of the period.

Shank is obdurate in his self-defense. As is often the case with someone unwilling to admit error or take responsibility for blunders, blame is cast on another’s shortcomings. In this case, the reviewer is faulted for failing to admire—as Shank evidently does—“over five hundred pages of densely worded text.” The text is indeed dense, but I strongly disagree that the “almost fifteen hundred footnotes” in any way support the claims that Shank makes. Not because I engage in any “partisan agenda,” but because the book is indeed “a tissue of errors” and “tententious” to boot.

Shank is correct on one point. Inexplicably, the word “French” was omitted from my citation of the book’s title. The lapse has nothing to do

with the content of the review, which focuses entirely on the French case study; to insinuate otherwise borders on mendacity. Having made this error, however, I draw comfort from Cicero’s maxim: “Cuiusvis hominis est errare, nullius nisi insipientis in errore perseverare.”

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TO THE EDITOR:

In his review of Kurt W. Beyer’s *Grace Hopper and the Invention of the Information Age* (Isis, 2010, 101:448–449), Thomas J. Misa makes one specific error, repeats two of Beyer’s errors, and, perhaps understandably, fails to note several other errors in Beyer’s account.

Specifically, Misa writes that Hopper received her Ph.D. in mathematics from Yale under Howard Engstrom (her advisor was Oystein Ore), when Beyer noted only that “Engstrom had been one of Hopper’s mathematics professors at Yale” (p. 35). Misa repeats Beyer’s mistakes that Hopper died at eighty-six (rather than at eighty-five) and that her divorce occurred in 1942 (rather than in 1945).

Our major concerns are not with Misa’s review but with Beyer’s numerous errors that appear to reveal a lack of attention to basic fact checking and adherence to scholarly conventions. One of Beyer’s most notable errors is his assertion, twice repeated, that “Hopper was the first woman to receive a mathematics degree from Yale” (p. 25). In fact, the first woman to earn the Ph.D. in mathematics from Yale was Charlotte Barnum, in 1895; furthermore, ten women had received such Ph.D.’s from Yale before Hopper received hers in 1934.

There are numerous other errors in Beyer’s account of Hopper’s life and career that are major deviations from fact and that would have been easy to check. For example, Beyer gives incorrect dates for Hopper’s leaving her position at Vassar College and for her reporting to the midshipmen’s school in Northampton, Massachusetts. Two other examples of incorrect statements are: “Hopper . . . consciously traded marriage and family for a career” (p. 5) and “Grace’s mother was an accomplished mathematician” (p. 25). He offers no evidence for these assertions, which, like the errors noted above, are specifically contradicted in sources he cites. This carelessness appears throughout the book in instances of quotations that are not exact, misspellings, and misuse of sources.

The examples noted above are just a few of